

NOTES FOR STUDENTS.

What is a species?—The many discussions as to what is a species have resulted in a general appreciation of the facts that species are not all of equal rank, that they are distinguished by more or less arbitrary characters, and that although many species are real natural groups of individuals, many others are simply arbitrary groups, associated for the sake of convenience. After reviewing the various methods of distinguishing species, KUPFFER⁴ concludes that no method will apply in all cases, that all methods are of importance, and that when the several methods are used conjointly, little difficulty is experienced.

KUPFFER then turns to the methods of KÖLREUTER, based upon the sterility of hybrids, as a method which has not been used to the extent its merits warrant. Sterility of the hybrids being presumably due to defective germ-cells, he depends for his measure of sterility upon the condition of the pollen, basing his method upon the fact pointed out a few years ago by JENCIC⁵ that viable pollen swells immediately upon the introduction of water, while the sterile pollen remains shrunken, and that this capacity of the normal pollen to swell is retained for many years in herbarium materials (more than 50 years in *Viola*, *vide* KUPFFER).

Although the author recognizes that considerable sterility of the pollen has been observed in many "good" species, he has himself never found a pure species in which more than a few (ein Paar) per cent. of the pollen grains remained shrunken, the implication being that the reported instances would bear further consideration.

After examining a number of species and their hybrids, especially among the *Violaceae*, he concludes that when a supposed hybrid shows much less fertility of the pollen than its supposed parents, it is not a *necessary* but a *sufficient* proof (1) that the supposed hybrid is truly a hybrid, and (2) that its parents belong to distinct species.

Application of this method is then made with interesting results to forms of *Potentilla*, *Viola*, *Thymus*, etc., which have puzzled the systematist.—GEORGE H. SHULL.

Propagation of grain rust.—Further comments by Dr. JAKOB ERIKSSON⁶ on the question of the origin and distribution of the rust-diseases of plants have recently been presented to the botanical public through *separata*. The author has not essayed so much to put forth new facts, as to bring together and review those recently published, in so far as they bear upon his mycoplasma theory, giving especial attention to adverse criticisms.

⁴ KUPFFER, K. R., Költreuters Methode der Art Abgrenzung nebst Beispielen ihrer Anwendung und einigen allgemeinen Betrachtungen über legitime and hybride Pflanzenformen. *Acta Hort. Bot. Univ. Imp. Jurjevensis* 6:1-19. 1905.

⁵ JENCIC, Untersuchungen des Pollens hybrider Pflanzen. *Oesterr. Bot. Zeits.* 50:1, 41, 81. 1900.

⁶ ERIKSSON, J. Zur Frage der Entstehung und Verbreitung der Rostkrankheiten der Pflanzen. *Arkiv för Botanik* 5³:1-54. 1905.

He maintains that after taking into consideration the studies and observations of MARSHALL WARD and PLOWRIGHT in England; MCALPINE and COBB in Australia; BOLLEY, HITCHCOCK, and CARLETON in North America; BARCLAY in India; KLEBAHN, DIETEL, SCHROETER, and MAGNUS in Germany; LAGERHEIM in Sweden, and others, the wintering of the uredo-bearing mycelium, or of the uredospores, so as to be a source of infection for the coming season, has not been proven. The evidence, chiefly as brought forward by KLEBAHN, to show that the first appearance of the rust in spring can often be accounted for by uredospores being carried long distances by the wind, is reviewed, and the conclusion reached that this is an assumption based on no direct evidence and highly improbable.

The author then enters upon the vital part of the subject and discusses the mycoplasma theory and its recent criticism, especially that which has been most ably presented by KLEBAHN and MARSHALL WARD. After an extended examination of the works of these authors, he finds that his theory has not been affected. He directs attention to a report by BIFFEN of recent experiments in hybridizing wheat carried on at Cambridge, England, in which the appearance of rust on the plants can best be explained by assuming that the mycoplasma of certain varieties was transmitted through the pollen to the resulting hybrid.
—J. C. ARTHUR.

Gynodioecism.—CORRENS⁷ has presented a second⁸ report on the gynodioecism of *Satureia hortensis* and *Silene inflata*, giving full confirmation of his earlier conclusion that the pistillate form produces only, or mostly, pistillate offspring when fertilized, as it must be, by the bisporangiate form. If the pistillate form is a mutant from the bisporangiate and differs from the latter by the possession of a distinct hereditary unit, as suggested by Burck,⁹ all the seeds produced by a pistillate plant are of hybrid origin, and the observed facts would be best explained as a case of dominance of the newly risen character over the older. In *Satureia* this dominance (?) is complete, but in *Silene* the offspring of the pistillate plants were pistillate in only 87–93 per cent., the rest being bisporangiate. Although this behavior looks very much like Mendelian inheritance, a number of cases are cited in which quite contradictory results have been obtained, so that while the author states it as a law that each sex has a *tendency* to transmit its own sex form, he does not look upon this as dominance in the Mendelian sense.—GEORGE H. SHULL.

An ear of corn.—The origin of such economic plants as wheat and maize, which have a wide distribution in cultivation but are unknown in the native

⁷ See BOT. GAZETTE 39:304. Ap. 1905.

⁸ CORRENS, C., Weitere Untersuchungen über die Gynodioecie. Ber. Deutsch. Bot. Gesell. 23:452–463. 1905.

⁹ BURCK, W., Die Mutation als Ursache der Kleistogamie. Recueil Trav. bot. Néerl. 1–2:95 sqq. 1905.